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Psychological Bulletin

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THE

PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES.

COMPARISON OF THE SEXES IN MENTAL TRAITS

BY LETA S. HOLLINGWORTH

Teachers College, Columbia University

To give this review a satisfactory title has not been altogether easy. It is the custom of the BULLETIN to publish from time to time summaries showing the results of experimental investigations in which the sexes are compared in mental traits, and the present review is intended to perpetuate this custom for the years 1916–1918. To entitle it "Sex Differences in Mental Traits" would lead the reader falsely to infer that all or most of the comparisons have shown differences. To call it "The Mental Traits of Sex" would imply that it discloses mental traits which are sex-limited. On the other hand, a title like "Sex Identity in Mental Traits" would be unfair, especially to such expressions of opinion as are to be included, which take the time-honored view that there are, and must be notable, inherent psychological differences between the sexes. Simply to adopt for a title "The Psychology of Sex" would give the erroneous impression that the review treats of literature pertaining to the sexual instinct. The title finally chosen seems to circumvent most of these difficulties.

From the standpoint of the experimental behaviorist no conclusions should be noted in a review under this title, except such as were based on the quantitative study of large numbers of both sexes, selected at random, or on exactly the same basis. And any difference found between the two groups thus selected and studied, could be announced as a sex difference only if it reliably exceeded the probable error of the average or median; or, being within the

probable error, was found constantly to occur in a large number of comparisons of similar groups with each other. Furthermore, a reliable difference thus found could be called an *inherent* sex difference only if it were shown to be present when the training and environment of both groups had been similar. The only point in pausing thus to state the obvious is that investigators sometimes neglect these principles of scientific method, and announce any difference found between two groups, arbitrarily segregated on the basis of sex, as a "sex difference." They forget that a chance difference would be found if the individuals composing the groups were re-segregated on the basis of any incidental factor—say eye-color or presence and absence of freckles.

The reviewer who would confine himself exclusively to results gained by the experimental method outlined above would, however, automatically tend to do himself out of his review. He would have very little to report. It was formerly a kind of convention among psychologists to include in the summary of results of a study where both male and female subjects participated, a paragraph on "sex differences." There seems now to be a growing tendency among those who have studied individual differences most extensively to omit this customary paragraph. For instance, Pintner and Paterson (14) make no reference to sex differences in their recent standardization of performance tests. On the basis of the extensive data from which his *Mental Survey* is derived, Pintner (16) states merely that "sex differences in these tests are too slight to justify separate norms for boys and girls." In the standardization of the picture completion test, Pintner and Anderson (15) say, "the test is equally well adapted to boys and to girls." Pressey and Pressey (17) postpone discussion of sex differences for a later paper.

Two comparisons of the sexes in memory tests have come to notice: E. F. Mulhall (13) tested 285 boys and 353 girls for recall and recognition, with various kinds of materials. She found that "for memory of words and syllables the averages are slightly higher for the girls, for forms slightly higher for boys," and that "there appears to be no marked sex difference in variability." A. I. Gates (5) from tests of adults concludes that women show slightly better performance than men in memory, and that men are slightly superior to women in reasoning. The amount of overlapping and the reliability measures are not given.

Marsh (11) presents results on two individuals, a man and a woman, who subjected themselves to a long fast, during which

mental tests were administered in order to detect possible changes in performance and feeling. "The sensory and passive sides of the self are not greatly affected, generally speaking, but sexually show male sensitivity for pain and perceptivity for dots increased, and for touch decreased; while for the female the reverse is true. Some improvement for both sexes is shown in mental clearness and accuracy, though not decisively; and a most pronounced effect upon the memory, disadvantageous for the male subject, and advantageous for the feminine. The feelings, usually acute for several days, and then usually apathetic for a time, were on the whole ambiguous indices of the grades of objective performance, less so for the male than for the female." What justification there could be for entitling such a study "sex differences" is hard to see, since it includes but one man and one woman, each of whom might easily differ as much or more from others of the same sex, as from each other.

The experiments of Berliner (3) with æsthetic judgments of school children, made on 180 girls and 180 boys of grammar school ages, show that "the ranking of a group of pictures is to a high degree the same for both sexes"; that "different groups of girls agree more closely in their æsthetic judgments than groups of boys"; that "the average positions of the pictures differ more from one another in groups of girls than in groups of boys"; that "inside the group the girls agree more closely in their æsthetic judgment than boys"; that "boys agree more closely in their dislikes than in their likes; girls agree more in their likes than in their dislikes,"—a result contradictory to results previously announced by H. L. Hollingworth and by E. K. Strong, Jr.; and that "the variability between the extreme and middle pictures tends to be greater in groups of girls than in groups of boys."

Rosanoff, Martin and Rosanoff (19) in extending the studies of free association previously undertaken by Kent and Rosanoff and by Rosanoff and Rosanoff, have made special comments in comparison of the sexes. They state that "all conditions being approximately equal, no difference is found in the showings of the two sexes, as regards our most significant measure,—the sum of 'high standard' values; but male subjects show very consistently a tendency to give either individual reactions or common reactions having no 'high standard' value, where female subjects furnish failure of reaction instead." They state also that "a tendency to give individual reactions or common reactions without 'high standard'

value, where normal subjects fail of reaction, seems to characterize neuropathic subjects, independently of sex."

Terman (21) and his collaborators have now published in monograph form the data from which The Stanford Revision of The Binet-Simon Scale is derived. Here the curves representing the performance of the two sexes are seen to be practically identical. No sex differences in variability appear. The conclusion of the authors is that the norms for general intelligence apply equally to both sexes.

Elizabeth E. Farrell (4) reports statistics from a survey of certain ungraded classes for children of exceptionally low mentality, in New York City. In these classes there were 258 boys and 103 girls. The investigator comments as follows: "The fact that more boys than girls are found in these ungraded classes permits of explanations other than that of greater variability in males. One of these is based on the fact that boys have greater freedom, are less restrained. Because of this they come into conflict with their school environment. This maladjustment makes it imperative that some notice be given to them, and some explanation sought." Race (17) selecting pupils for an ungraded class of superior children in a city school system, found 10 boys and 11 girls who were eligible on the basis of exceptionally high intelligence, and physical fitness.

In the field of animal psychology Bagg (2) has contributed a quantitative study of individual differences in the performance of white rats, which reveals no sex differences.

Turning from conclusions based on quantitative data, we wish to note recent expressions of theory and opinion, bearing on the behavior of the sexes. These are mainly discussions of the psychological considerations involved in the changing social status of women. Hull (7) has published an article pointing out the implications of psychoanalytic concepts in the study of the behavior of women and girls. The concept of hysterical and irrational reaction as the result of chronic blocking of strong, fundamental conations, by taboo, social suppression, economic nonentity, and the like, is brought to the attention of those who would understand the efforts of women to find another "place." In this same vein are chapters of Adler's work (1), published some years ago, but not translated into English until recently.

Moxcey (12) has written a readable book, based with discrimination on the data of psychology, and intended for those whose work has to do with the guidance of adolescent girls. L. S. Hollingworth

(6) has advanced the theorem that the traditional division of labor between the sexes implies no sex difference in mental traits; that it is to be explained solely on the basis of sex difference in the physiological function of reproduction, which has always bound the female to the house and offspring, leaving the male relatively free vocationally; that the traditional division of labor is what we should expect to find, even though there were no sex differences in mental traits.

Russell (20) expresses the belief that the concept of maternal instinct has been greatly overworked, in the service of social control, and that women will become increasingly resistant to exaggeration of its importance as a determinant of their careers. Jastrow (8) is convinced that "the feminine mind" is a type, different qualitatively and quantitatively from "the masculine mind," and has again presented his opinions in a chapter of his recent volume, *The Psychology of Conviction*. Wells (22) occasionally implies the existence of sex differences in instinctive equipment, as when he discusses work trends and mastery trends in connection with the masculine pronoun, and parental trends in connection with the feminine pronoun. On the whole, however, the reader is left to infer that the mechanisms of adjustment work independently of sex.

It is quite certain that not all the literature which compares the sexes in mental tra ts, and which would deserve comment, has been seen by the present reviewer. For example, publications have appeared under titles which would imply that they contain such comparisons, by Lipmann (9) and by Lombroso-Ferraro (10) respectively, but they have not been accessible to the reviewer. Now that the war is finished, foreign contributions will doubtless begin to become available once more. They are conspicuously absent from this report.

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THEORETICAL ETHNOLOGY

BY ROBERT H. LOWIE

American Museum of Natural History

The theory of a worldwide diffusion of cultural traits, which Graebner brought so prominently before the ethnological public about a decade ago, has been most vigorously championed of late years, though in a different form, by the British anatomist Elliot Smith, who has founded what may be called the Manchester school of anthropology. His most recent publication (**9**) presents a complete sketch of the origin of human civilization and an exposition of the psychological factors that operate in its evolution. The principle of psychic unity which has played so prominent a rôle in anthropological speculation is definitely abandoned. All cultural similarities must be the result of borrowing, for the phenom-

ena of civilization are regarded as so artificial and arbitrary that, failing specific human instincts for their creation, they could have sprung into being only once by sheer accident and then been disseminated over the globe from this place of evolution. The great center of dispersal, according to Smith, was in Egypt or at least in its immediate vicinity. More particularly are the features of New World culture derived from the ancient civilizations of the Old World. While these views have found but little acceptance among professional ethnologists, outside of his immediate entourage, the tremendous influence which Smith ascribes to the forces of tradition rather than to hereditary factors in the history of culture cannot fail to strike a responsive chord.

It is indeed from appreciation of the power of tradition that ethnologists continue to issue declarations of independence against other branches of learning. Rivers, for example (7), denies that psychology can furnish principles of interpretation to the student of social phenomena in the sense that physics or chemistry can supply such principles for the geologist's use. Answering Westermarck's objections that such a social phenomenon as the blood-feud can be explained only by the psychological phenomenon of "revenge," Rivers points out that in Melanesia this interpretation breaks down, and in the case of head-hunting purely religious sentiments take the place of a desire for retaliation. In order to understand the individual Melanesian's reactions it is necessary to understand his social environment. "In place of asking, How can you explain the blood-feud without revenge? I would rather ask, How can you explain revenge without a knowledge of blood-feud? How can you explain the workings of the human mind without a knowledge of the social setting which must have played so great a part in determining the sentiments and opinions of mankind?" From a somewhat different angle the autonomy of cultural studies is reasserted by Kroeber (5). The development of civilization out of merely biological processes represents a saltation to a different plane, that of the "superorganic," which henceforth evolves independently of any organic substratum and can no longer be investigated by the methods of mechanistic natural science.

A *rapprochement*, though hardly as yet an *entente cordiale*, is recently noticeable between psycho-analysts and anthropology. Freud's articles on incest rules and various social observances among primitive tribes, which originally appeared in *Imago* (1912–1913), have been assembled in book form and during the past year

published in English guise (1). Although the interpretations given are hardly palatable to the ethnologist, the volume contains a lucid exposition of two important concepts, the Œdipus motive and ambivalent emotions, and may stimulate the independent testing of their validity in the light of ethnographic data. While Freud approaches these anthropological facts from the point of view of the medical practitioner, Rivers has the unusual qualifications of a trained physician who has specialized for years in ethnological research and has latterly had occasion to apply psychoanalytic treatment in connection with war work. In a paper already reported on from another viewpoint (7) he draws attention to the great importance of the subconscious in social life. There is here a distinct parallelism between the individual and social mentality inasmuch as ostensibly introspective statements as to motives of thought and action represent nothing but an intellectualistic gloss over the real unconscious sentiments involved: they are rationalizations or, as American ethnologists would say, secondary interpretations. Quite recently he has published a lecture especially devoted to analogies between dream life and various manifestations of the cruder cultures (8). This essay differs from corresponding attempts by the Vienna and Zurich schools in that attention is not confined to a consideration of myth but embraces various other aspects of culture. Thus, symbolization is illustrated by the representation of an absent child by a coconut and this case likewise parallels the dream process of condensation, since a whole series of associated ideas is stenographically represented by the fruit. In corresponding fashion Rivers finds analogies for displacement, dramatization, and secondary elaboration. The upshot of the argument is summed up in the statement that the mechanism by which the dream-thoughts find expression shares its general characters with those which have produced the rites and customs of savage man.

In a very different sense a connection between ethnology and psychology is attempted by Haeberlin (3), who demands that the principles of primitive art be studied "by methods comparable to those applied in the æsthetics of our own." Hitherto the stress has been placed too largely on matters of ethnographic moment. The t me has come to view the primitive artist not merely as a type but as an individual, to inject the biographical factor into this line of research.

Goldenweiser (2) has materially modified the conception of

totemism outlined in his previous contributions to the subject. While he was formerly inclined to minimize the content of totemism and to emphasize the formal factor, he now insists on the specific content. The distinctiveness of the phenomenon lies in the association of this content with a clan system, which differentiates it from the otherwise comparable phenomenon of religious societies. Totemic complexes uniformly "partake of that specifically socialized supernaturalism which is particularized in the varying clan contents."

Another subject of perennial interest relates to the processes of social evolution. Hartland (**4**), in an attack on Swanton's and Lowie's views, has taken up cudgels on behalf of the traditional theory that patrilineal descent has been uniformly preceded by a system of matrilineal reckoning. This conclusion he tries to establish by examining African, American, and Australian data with the aid of the method of survivals. In a rejoinder (**6**) Kroeber points out the North American data which have led cis-Atlantic anthropologists to argue in favor of a very different sequence from that postulated by the classical school. According to the American point of view a loose form of organization preceded exogamy, and patrilineal reckoning of descent was prior to matrilineal institutions. This is borne out by the fact that the lowest tribes culturally are uniformly non-exogamous, while the agricultural peoples almost regularly share a clan system.

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PSYCHOPHYSICAL MEASUREMENT METHODS

BY HENRY A. RUGER

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Cowdrick (3) compares the Weber-Fechner law and the function of Fullerton and Cattell as to closeness of fit to data secured for the Sanford weight experiment. In general the experimental data fall between the two sets of values found by means of the formulæ. 89 records made by 48 observers were utilized. 46 of these records were made with no previous practice, the remaining 43 followed 1, 2, or 3 sortings. In the "no practice series" the Fullerton-Cattell formula gives the better fit. In the series which followed practice the Weber-Fechner law gives results closer to the data. Goodness of fit is defined in the sense of the method of least squares, and the magnitude of the sums of the squares of the deviations is used as the measure of closeness of fit. The fit is poor by either formula if the low terminal point of the four equal sense distances is included. The importance of studying the validity of the Weber-Fechner law with longer practice series is stressed. The data utilized were Cornell Laboratory records ranging from 1906 to 1917.

Hoisington (7) studies the effect of practice on the two point limen and upon its precision. The experiental data were secured by A. M. Palmer and P. R. Dickinson, who entered the national Service. 200 series were taken on each of two subjects. The 200 series were divided into successive groups of 25 series each, of 50 series each, and of 100 series each. No practice effect was discernible for the limen, but there was an increase in precision. Fifty series sufficed to give a difference between the limens of the two subjects with "96 per cent. probable correctness." In the case of one of the two subjects the closeness of fit of the Phi-Gamma function increases markedly with practice.

Langfeld (8) investigated the finger-span limen by means of special apparatus consisting of 6 Starrett calipers with suitable mounting and accessories. The six subjects were given a preparatory training, involving about 700 judgments each, in order to reduce the disturbing effects of practice. The method of constant stimuli was employed. Judgments of "larger, smaller, and equal" were utilized and degrees of confidence of "*a*, *b*, or *c*" were accepted. Doubtful, no-difference, and or-judgments were classed as of con-

fidence "*d*." The subject was forced to make guess judgments in the case of the "*d*" judgments, but they were not used in the main computations. The phi-gamma hypothesis was employed, the constants being determined by the method of least squares. The measure of sensitivity was found to be about 1 per cent. of the standard distance for thumb and forefinger, that distance being 5 cm. Successive presentation of standard and comparison stimuli to one hand, the right, gave results slightly superior, perhaps, in sensitivity to successive or simultaneous presentation to both hands. There was a positive association between right and wrong judgments and degrees of confidence. Right answers predominated in the case of forced judgments. The forced judgments were associated with a passive, involuntary attitude. The investigator recommends that subjects of psychophysical experiments be trained in this attitude.

Boring (1) gives a graphic and verbal elucidation of the relation of the phi-gamma function to the curve of error for the benefit of elementary students. For purposes of simplification a binomial expansion of low degree is substituted for the probability integral.

Fernberger (5) replies to Boring in the discussion concerning the number of observations necessary for the determination of a limen.

Burtt (2) studied the illusions of movement resulting from two successive tactual stimuli. Blunt pointed brass rods controlled by solenoids were used as stimuli. By means of a time sense apparatus and suitable resistance, the duration of application of the points, the interval between applications, and the intensity of application were controlled. Four subjects were used. They were not informed as to the expected result. A control for the perception of movement was provided. The results correspond in a very interesting and suggestive way to those of similar experiments with visual and auditory stimuli. A tendency to reversal of the illusion is found to exist when the second stimulus of a given pair is the more intense. The illusion seems to be most pronounced when the relation between the distance apart of the two stimuli and the length of interval between them is direct, *i. e.*, if greater distances accompany longer intervals. The optimum relation between intensity and distance is also direct, but that between intensity and length of interval is reverse. The investigator finds confirmation in these results both for the central origin theory of the illusion and for his own action theory.

De Camp (4) concludes from his experiments that there is is only a relatively slight influence of color of objects upon their apparent weight. Hue is considered to be effective independently of tint value. The apparent weights of a series of differently colored blocks of equal physical weight were obtained by three different procedures. These were (1) the method of paired comparisons, (2) direct serial arrangement, and (3) the selection of an equivalent weight from a graded series of weights all of a single gray. The agreement of the different methods was not close. The experimenter expresses preference for the method of paired comparisons on general grounds, but does not refer to the experimental studies bearing on the point. On the basis of results secured by the method of paired comparisons he states that there is a tendency to judge a red or black object to be slightly heavier than a yellow or blue object of the same weight. While the reliabilities of the differences previously mentioned are apparently very slight, the differences between apparent weights when sight only is used seem to be well established. Blue and black are judged to be the heaviest, yellow and white lightest, red and gray to be intermediate. There is no apparent relation between the affective value of the different colors and their weight as estimated by lifting.

Curtius Müller (9) presents an interesting paper, mainly historical, on errors of observation in the estimation of subdivisions of the scales on geodetic instruments. The writer lists a number of problems demanding investigation and asks for the cooperation of psychologists and physiologists in their solution. Müller reports an experimental study of his own on the relation between errors of estimation and the size of the scale intervals employed. The distances to be measured were the intervals between lines scratched on a metal plate. 8 scales with subdivisions giving field dimensions from ½ mm. to 100 mm. were employed, the divisions of a single scale being uniform. Each of 8 observers gave 256 judgments. The subjects were chosen at random from students of geodesy and included some experienced observers. A comparator was used to check the accuracy of the judgments. The judgments were estimates of subdivisions of the finest intervals on a given scale. For fields varying in size from 1 mm. to 100 mm. Müller found that the errors of observations were on the average approximately proportional to the field size, *i. e.*, the relative errors were constant in magnitude. The relative error was higher with a field of ½

mm. Eye strain and mechanical difficulties prevented the use of smaller fields.

Grabfield (6) reports a study on another subgroup of psychopathic subjects. The faradic limen by the Martin method was determined for thirty cases considered to be in the psychoneurotic group. In general the subjects exhibited normal æsthesia, some may have been hyperæsthetic. The finding is expected to have diagnostic value in differentiating this group from other groups which show an hypæsthesia to faradic stimulation. Some cases of traumatic neurosis and occupation neurosis did not exhibit the tendency of the psychoneurotic group as a whole. It is to be hoped that Dr. Grabfield will publish his complete raw data for all of his investigations or the reliabilities of the differences between the various groups he has studied.

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SPECIAL REVIEWS

Les Émotions et la Guerre. Réactions des individus et des collectivités dans le conflit moderne. M. Dide. Paris, Alcan. 1918. Pp. 276.

This book, written in the trenches and in neurological centers at the front and in the interior by the author of *Les Idéalistes passionnés*, brings together experiences and reflections of considerable

interest and value. It is divided into three parts, the first treating of war shock, the second of the egoistic, and the third of the altruistic types of war psychosis. Throughout the author aims at showing the superiority of affective forces over rationalistic constructions and in the first two parts contests the view that organic lesions are responsible for such mental disorders as he claims to be curable by psychotherapy.

One notes at the outset his definition of emotion as related to affective tone. By affective tone he means, not the simple algedonic qualities, but that state of euphoria, plenitude, or, on the other hand, of insecurity, apprehension, which the individual possesses, independently of any actual stimulation, as a sort of personal and hereditary equilibrium. Emotion is defined as "the intimate reaction of the individual with regard to the solicitations which tend to break up this mean affective tonality." This opens a wide field of application.

Dealing with the physical effects of explosions the author is of opinion that they rarely, if ever, occur through concussion of the air alone outside the zone of bursting material. This he confirms by his personal experience and by observation of cases following heavy bombardment. Apart from physical injuries physically produced shell-shock is functional and probably never the cause of permanent disorders of the nervous centers. Cases which appear to favor the contrary opinion are found on examination to be susceptible of interpretation.

Not all mental disturbances occurring during war arise from its operations and fatigues. The psycho-emotional reactions that do thus arise are psychic inhibitions and connected disturbances of memory. Psychic inhibitions are distinguished by either a minimum or a complete syndrome; the latter shows various types, the most pronounced being characterized by onirism reproducing most often the tragic or horrible scene which immediately preceded the hypnogenic shock. In both the lighter and the severer cases cure is effected by the breaking up of the actual emotion and the substitution of other emotions, or by setting free a desire, hope or passion. In general the methods employed are those of the professional hypnotist, aided at times by a strong dose of galvanization substituting a physical pain for the hypnogenic emotion. The disturbances of memory are distinguished as amnesias of fixation and amnesias of evocation; in the former attention is fixed on the memory of the tragic experience, in the latter all or part of the life

at the time or before the accident is forgotten. The *amnésies parcellaires* are the most curious. Prior to the war, we are told, a few cases were reported and admitted with some reserve; there is now no doubt of the facts. Several cases are cited.

Taking up the theories of battle psychoses the author discredits the "commotional"theory, with its assumption of organic disturbance, for cases where the phenomena exhibit only temporary interruptions of function. He also doubts the universal applicability of the strongly supported theory of autointoxication accompanying traumatic oniric confusion; he inclines to think that reflexes of a purely psychological character usually suffice to explain the observed disorders, without, however, denying the possibility of secondary toxic infections, or even of authentic toxic mental confusions. He approves the theory of emotional shock first formulated by Ballet and de Fursac in 1916 and adopts the view which assimilates the consequent phenomena—psychic inhibition, hyperemotivity and hyper-activity of imagination—to the phenomena long since observed and described under such terms as hypnosis and somnambulism. He cites his own personal experience in confirmation. He regards the hypnosis produced by emotional shock—and this occurs not only in war, but in other great catastrophes as well—as genetically a defence reaction, like natural sleep, whose function, according to him, is primarily prophylactic against exhaustion and fatigue and only secondarily for the elimination of toxins; it is comparable to the so-called "death shamming" of animals, but is ill adapted to the conditions of modern warfare and found chiefly among novices.

But war psychoses are not only those of shock; besides the hypnotic type, there is the hysterical type and there are other allied types. Dide uses deliberately and, as he claims with definite meaning, the abused term hysteria. It imports, according to him, a fixed affective tendency, but in a manner subconscious or unconscious, favoring a dissociation of the personality and characterized mainly by unconscious instability of affectivity. He enters a plea for a return on this subject to the classic French psychology as over against the school of Kraepelin. Of hysterical psychoses having direct relation to the emotion of war cases of the recidivist type, the permanent cure of which is always doubtful, are distinguished from the rarer cases in which the hystero-emotional psychosis is prolonged. The characteristic of war hysteria is the absence of affective systematization. The permanence of the

symptoms is due to the fixity of the subconscious element which supports it. Break this up by a substituted emotion, and the other symptoms—amnesia, dysmnesia, the tendency to live on dreams, etc.—disappear. These cases, however, must be carefully distinguished from others in which analogous manifestations are sustained by a defective intellectual factor or by an egoistic systematization due to tendencies of self-justification (*revendication*). This psychological method of discriminating physiopathic and hysterical disorders has interesting practical and therapeutic consequences.

The sustaining element in many cases in which there is no organic injury but which do not yield to suggestion is the cowardice of the patient, his aversion to pain, and to this is usually added congenital mental debility, which is quite compatible with a certain intuitive finesse, and this debility is often supported by a syndrome of motor debility. Thus an arm held motionless for a long time remains paralyzed, a limb or some part of the body which has assumed a special attitude to relieve the pain remains contracted. Paralysis or contracture thus induced may affect the motor area of the cortex; steady prolonged suggestion approaches in its effects the sudden amnesic or dysmnesic phenomena of hysteria. This variation blends with the *constitution revendicatrice* familiar before the war; the soldier thinks he has done enough and should now be allowed to retire, and in this he commonly receives the sympathy and support of his friends. With this passional egotism hysterical psychopathy may incidentally mingle. Dide discusses in detail the two main features of these physiopathic syndromes, the fixed antalgic attitude and the trophic and vaso-motor disturbances, emphasizing in regard to the latter his view that a phobia of pain, and sometimes a state of mental and motor debility, favors functional incapacities which suggestion, created by a habit, systematizes. Moreover, if immobility (*e. g.*, of an arm in a sling) produces ankylosis of the articulations, the concomitant inertia produces an ankylosis of the mental activity.

Finally, there is war neurasthenia. Many symptoms have been wrongly attributed to local lesions which really depend on a diffuse reaction identical with that connected with emotion. The mechanism is different from that in hysteria, different, too, from that of the *revendicateur* whose egotism withdraws him from danger, but a more passive and diffuse yielding leads to essentially the same result. Neurasthenia may be constitutional and start spontaneously, but moral traumatism may become a proximate cause with a minimum

of predisposition. Contrary to what might be expected Dide found in his own experience only a small proportion of hereditary and personal degeneracy among those suffering from great emotional disturbances due to the war. The conditions of war are so different from those of ordinary life that they can to a large extent modify ancestral tendenceis. A normal individual may appear degenerate, an unstable degenerate may become a hero. Moreover, emotion is not the sole factor in acquired emotivity; overexertion is also a factor, and the case is cited of an officer promoted to a high command at Verdun who signed a paper testifying to his own incompetency after severe and conscientious labors. The neuro-psychic asthenia of war approximates all the manifestations of cortical fatigue. Emotion plays a capital rôle, a special predisposition favors its expression. The cardiac affections often found in these cases cannot be assimilated to those of hysteria, but are due to exhaustion and emotional causes. Those associated with head-wounds depend, it is held, not on the cranio-cerebral lesion, but on a post-traumatic neurasthenia.

A chapter of special interest to the practitioner, but of less immediate importance now that the war is over, concerns the way the functional troubles of war should be treated. It warns against false cures and the mutability of the symptoms, urges speed and decision of treatment in a suitable milieu, that is, near the front and not in the interior, and in general follows the lines of the psychotherapy of the French neurological school which, the author says, has made it possible to return a large proportion of the mentally injured to the army. This part of the book concludes with a characteristic French diagnosis of the collective psychopathic criminal egotism of Germany.

The third part treats of the altruistic passions. Egoistic and altruistic sentiment, both individual and collective, have a common origin in the instinct of conservation, the one negative, pessimistic, the other expansive, optimistic; to the need of perpetuating his physical being man adds the need of moral superfluity, luxury, irradiation. Most important for its influence on expansive tendencies in the troops is the affective psychology of the officers in command; the officer must inspire enthusiasm while preserving full lucidity, he is the reflex center of regulation for his unity. But his mentality is not rigidly confined to one type. The expansive tendencies in the troops are made up of many elements, such as the sense of discipline, esprit de corps, songs and laughter, the sense of duty to the collectivity, patriotism, religion, hatred of the enemy,

which together combine to form a social instinct of expansion sufficiently powerful to repel the solicitations of the instinct of conservation. These factors operate variously. For example, the religious sentiment is said to be rarely dominant, yet it is undoubtedly an aid to the believer in the performance of his military duty; but among simple souls the question is oftener one of superstition, connected with blessed medals, scapularies, etc., than of faith. As to hatred of the enemy, it is more a negative sentiment; the soldier fights to reach the assigned objectives and not from hatred of the soldier opposed to him; but victorious, he is capable of every violence.

Heroism reveals an affective tendency elevated to the sublime. The author's discussion is based on accredited facts, but runs out into somewhat vague speculation. He finds that the heroic act commonly represents an irresistible activity, sometimes an inhibition, *e. g.*, suppression of all external manifestations of pain and grief; at times the heroic attitude is sustained through a series of decisions and acts in which the instinct of conservation is in conflict with that of sacrifice; then, again, in certain high examples, tragic renunciations are involved. The impulsive type is found on occasion associated with degenerate impulsive tendencies, a case in point being cited; with regard to heroic inhibitions, the question is raised whether they may not be related to the phobias. Heroic impulsions that are not degenerate are often found among the simple, the heroic inhibition is habitually the appanage of men of rank and culture. The irresistible quality in the impulse is analogous to that of the suggestion in an imperative command; "our instincts are the resultant of suggestions of an infinite past." The speculative conclusion finally reached is that heroism is derived from the genital instinct, whereby it is also related to æsthetic and religious inclinations, which, according to the author, have a similar origin. "At the basis of all sublime renunciation exists a little of the *amour feminin* and often even the unconscious tendency to render oneself worthy of all desires." The author goes so far as to suggest an analogy between the sexual act which assures the perpetuation of the race and the self-transcending sacrifice of the warrior impelled by the forces of destiny and symbolizing the nation's need of life.

A final chapter, parallel to that on the egotism of Germany, gives the author's interpretation of the altruistic motives inspiring France and her Allies.

H. N. GARDINER
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An Enquiry into the Analytical Mechanism of the Internal Ear. THOMAS WRIGHTSON. Appendix on the Anatomy of the Parts Concerned, by ARTHUR KEITH. London: Macmillan, 1918. Pp. xi+254.

This is an important contribution to the theory of the functioning of the internal ear, and one which appears to administer the *coup de grâce*, if such were wanting, to the long held resonance theory of Helmholtz. While disproving on grounds of mechanical theory and anatomical discovery that resonance is the principle that underlies the stimulation of the auditory nerve receptors, the present theory is not founded on the idea of the telephone receiver. The idea which it promulgates and attests is rather one of *displacement*, as made familiar to psychologists by Max F. Meyer, and to some extent by ter Kuile. Although the complete presentation of his views bears a recent date, we are informed that Wrightson's contributions on the subject were begun over forty years ago. For full understanding of the painstaking investigations instituted and carried through by the author during these years of patient inquiry, as well as for the anatomical corroboration furnished by the illuminating research of his collaborator, the reader must be referred to the book itself. Here we may content ourselves with a brief statement of the theory.

The improvement over Meyer's conception of displacement is attributable largely to Wrightson's discovery that a single pendular wave affords four distinct stimuli to the auditory mechanism. These correspond to four phases of the wave-form: first at its rise, second at its crest, third at its point of crossing the base line (nodal point), and fourth at its trough. With the aid of a mechanical device for plotting compound curves Wrightson demonstrates by a method of counting that if we take these four phases into consideration, there occur periodic changes to account not only for the simple tones, in accordance with Fourier's law, but also for the combinational—both the summation- and the difference-tones. Transferring this complex of stimuli to the internal ear, it is shown that the basilar membrane responds in like fashion to occasion in the cilia of the nerve cells *transverse* movements corresponding to the four phases with periods of quiescence between them. The movements of these hairs set, according to Keith, in the jelly-like substance of the *tectoria*, furnish a discretely tactual mode of stimulation to the nerves which may in turn be synthesized or analyzed by the cortex.

The theory appears to be adequate for all the chief points on which the Helmholtz theory has relied for its support. The mechanical principles are worked out with refined nicety by Wrightson's expert engineering technique, while Keith's experimental research upon the anatomy of the organ makes clear its evolutionary adaptation for the purpose at hand. The only serious omission that a student of recent psychological literature on hearing may detect at a cursory view, is an explanation of the attributes of auditory *volume* and *brightness*. Both authors explain the gradual widening of the basilar membrane towards the apex of the cochlea as adapted for lessening the resistance to intensive impulses. Keith, at least, recognizes that high tones are more specifically involved in the action of the basal end of the membrane, while low tones bring more extended portions into play; but the reason of this might be made clearer. The possible association of wider stretches with increased volume, and the brightness of pitch in tones, as compared with the dullness or roundness of vowel-sounds, are subjects the authors do not consider. Yet these and similar questions are not without a promise of solution within the scope of this theory. Certainly we have here an investigation of the problem which is both profound and highly suggestive. The book may therefore be heartily commended to all who interest themselves in the perplexing questions of auditory theory.

R. M. Ogden

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War According to Clausewitz. Edited, with commentary, by Major-General T. D. Pilcher. London: Cassell, 1918. Pp. 257.

This is a rather free translation of the first and most important work of Clausewitz, who died in 1831. It discusses the nature and theory of war, strategy in general, and finally the combat itself. It is a far broader work than Berhardi, and, while it stresses greatly what might be called the mechanics of war, it lays far more emphasis on morale than do most recent German writers.

G. Stanley Hall

Le Courage. L. Huot & P. Voivenel. Paris: Alcan, 1917. Pp. 358.

This is the broadest and most comprehensive treatise on the subject, its history, literature, manifestations in war and its psy-

chology, and sketches with great detail the inner history of a great conflict, its beginning, acme, and end. At the apex of his excitement the fighter's state is masochistic and he absolutely loses fear. There are other analogies between the erethism of war and that of sex. The author's main thesis is that courage is the triumph of the instinct of social over individual preservation. It abounds in very acute observations.

G. S. H.

France and the Next War. A French View of Modern War. J. COLIN. London: Hodder & Stoughton, 1914. Pp. 316.

Like nearly all the works of French writers everything here centers from the battle itself. This is a careful psychological sudy, especially of the Napoleonic wars, stressing morale from the standpoint of the battle.

G. S. H.

Le Combat. GENERAL PERCIN. Paris: Alcan, 1914. Pp. 301.

This book begins and focuses in the combat itself but describes the different kinds of fear in the various arms of the service, and in the last chapter moral forces, both material and intellectual to the advantage of the latter.

G. S. H.

The Principals of War. F. FOCH. (Trans. by J. de MORINNI.) N. Y.: 1918. Pp. 372.

Here we have the principles of Foch the Teacher which he has lived up to. The whole work is sown with references to morale, which is the force that most needs to be economized, that is regulated by intellectual discipline, that is affected by strategy. The last three chapters culminate, like all French works, in the battle itself.

G. S. H.

Hier et Demain. G. LEBON. Paris: Alcan, 1918. Pp. 252.

In this work the author applies his psychology of peoples and the crowd to war before and during battle, and seeks to give a practical application to his view that the force of the army is the force of collectivity, a view that underlies both his *The Psychology of Peoples* (N. Y., 1912, 216 p.) and his *Enseignements Psychologiques de la Guerre Européenne* (Paris, 1916, 364 p.).

G. S. H.

The Biology of War. G. F. Nicolai. New York: Century, 1918. Pp. 553.

The author of this book, which is one of the very best the war has produced, was formerly professor of physiology in the University of Berlin, and suffered bitter persecution at the hands of the German government for printing his valuable work. Part I discusses the war instinct, war and the struggle for life, selection by means of war, the chosen people, how war is metamorphosed and the army transformed, the roots of patriotism, its different species, unjustifiable chauvinism, the legitimate individualism of nations, and altruism. Part II tells how war may be abolished, describes the evolution of the idea of the world as an organism and how this conception has been voiced, or rather how unsuccessful have been the attempts to express it, discusses the transformations of human judgment, and finally war and religion.

The author starts with a drastic arraignment of the ninety-three German professors who signed the famous German Manifesto of October, 1914, which prompted his book. He shows remarkable familiarity with the history of war, but the chief thesis with which his book concludes is that God is humanity, theology is anthropology, and in this way he redefines in modern form the conception first set forth by Feuerbach that all modern conceptions of God are really those of humanity ejected and projected upon the clouds. God is Man and therefore brotherhood and peace must evict war.

G. Stanley Hall

Morale. H. Goddard. New York: Doran, 1918. Pp. 118.

This is largely a reprint of articles but a most stimulating book for soldiers. The preliminary morales are health, gregariousness, and humor. The major are pugnacity, adventure, work, communal labor, justice; while the composite morales include pride, victory, sport, fatalism, and reason. Then comes the supreme morale, which is that of creation. Sex and Morale and Morale and Reconstruction are also included. G. S. H.

Morale and Its Enemies. W. E. Hocking. New Haven: Yale Univ., 1918. Pp. 200.

The author was at the front for a short time during the summer of 1917. The substance of this book was given in lecture courses. The first part treats of the Foundations of Morale, and includes

chapters on why morale counts and how much, what is good morale? its foundations—instinct and feelings, knowledge and belief, realizing the war, enmity, the purposes of Potsdam, the mote in our own eye, and state blindness. The second part deals with the Morale of the Fighting Man. Here are chapters on the psychology of the soldier, discipline, will, practice, command, morale-building forces, fear and its control, war and women, and the longer strains of war. G. S. H.

The Psychology of Courage. H. G. Lord. Boston: J. Luce, 1918. Pp. 164.

The author is a professor at Columbia University. His book deals with mechanism in man, the nature of courage, its simpler and lower forms, acquired and complex mechanism in its higher forms, courage of differing patriotisms, its ultimate foundations, training—general and special, restoration of courage when lost, shell-shock, and an epilogue on morale. G. S. H.

Making a Soldier. Major-General W. A. Pew. Boston: Badger, 1917. Pp. 220.

This book consists of lectures given informally at the monthly conferences of the Training School of the Massachusetts National Guard. The chief topics treated are discipline, knowledge and ideals, interest, the struggle, habits, instincts, pugnacity, education, play, self-assertion and self-abasement, gregariousness and fear, preparedness and the militia. This is a very vigorous, stimulating, and practical book. G. S. H.

The Psychology of Handling Men the Army. J. Peterson & Q. J. David. Minneapolis: Perine Book Co. Pp. 146.

The junior author has had much experience, and the book treats mainly of competition, play, team-play, leadership, principles of learning, health, discipline, and loyalty. The book was submitted to the War Department, which authorized its publication. G. S. H.

The Psychology of War. J. T. MacCurdy. London: Heinemann, 1917. Pp. 68.

This treats chiefly of primitive instincts and gregariousness and its correlation with primitive instincts. The author has made very important contributions in the base hospitals to the knowledge and treatment of shell shock. G. S. H.

Morals and Morale. L. H. Gulick. (Introd. by R. B. Fosdick.) Association Press, 1919. Pp. 192.

This book was practically finished before the author's death, and has been brought down to date by the most competent of all authorities. Dr. Gulick studied the sex problem at the front, and the last half of his book is made up of appendices, starting with the messages of President Wilson and Secretary Baker and containing the important documents which show just what our government has done for sex in the army. This is the best and most comprehensive work on the subject. G. S. H.

Les Etudes sur le Combat. A. Du Picq. Paris: Hachette, 1880.

Until Marshal Foch's book appeared, this has been probably the most characteristic presentation of the psychology of the actual face-to-face combat, which the French make central in their war theory and teaching just as the German works tend to center about maneuvers and tactics. G. S. H.

Psychology of War. LeRoy Eltinge. Fort Leavenworth, Kans.: Army service schools, 1915. Pp. 126.

This is a very effective book and widely read by officers, based to some extent on LeBon's principles. The psychology of the crowd and mass is discussed, and there are excellent chapters on panic in war and on the psychology of infantry combat. In an appendix he discusses the causes of war, which bottom on the increase of population and economic pressure, and this, to the author, shows that war is inevitable. G. S. H.

DISCUSSIONS

A NOTE ON VISION—GENERAL PHENOMENA

We wish to make the following corrections of Dr. Troland's review of work published by us during the year 1917–1918.

1. He says of our work on "The Power of the Eye to Sustain Clear Seeing under Different Conditions of Lighting": "Semi-indirect reflectors of high density seem to be most conducive to eye comfort." While this conclusion would perhaps be more agreeable and satisfactory to certain commercial and professional factions in lighting circles, it was not drawn by us nor can it be drawn from our results. Of the commercial reflectors tested by us thus far, unmodified by any experimental device for the improvement of their effect on the eye (cf. Opaque Direct Reflectors, *Trans. of Illum. Eng. Soc.*, 1917, **12**, pp. 466–468), the best results have without question been obtained with the totally indirect reflectors.

2. Of our article, "Some Areas of Color Blindness of an Unusual Type in the Peripheral Retina," he says: "Ferree and Rand report observations which show that areas can be found in the peripheral visual fields of many persons which are relatively blind to red, green, yellow or blue but which are not correspondingly deficient in the complementary after-image and other related reactions." There is no ground for using the term "relatively blind" here. It was shown in the article reviewed that stimuli so intense as to carry the sensitivity to red, yellow and blue out to the limits of white light vision were not sensed as color in these areas. Further in order to leave no doubt as to their color blindness the series was made to include also stimuli so intense as to give colors greatly reduced in saturation when viewed in central vision. If areas so tested are to be called "relatively blind" it is difficult to understand why the term color blind should ever be used. What we actually reported was that both types of areas, color blind and color deficient, showing no detectable loss in the cancelling and after-image functions were to be found in the peripheral retina.

3. Our discussion of the "Needs and Uses of Energy Measurements in Psychological Optics" is represented in a way which we do not care to have stand uncorrected in the year's reviews of work. Our argument was that if we are to determine the sensitivity of the eye in a way that is comparable with the determination of the sensitivity of the physical recording instruments we should be able to compare numerically both our amounts of response and

amounts of stimulus. The question whether stimuli should be equated subjectively or in energy terms for investigations bearing on various points of theory or doctrinal conception was quite aside from the main purpose of the paper. Which type of equation should be used depends on the nature of the problem in hand, as was made clear as a feature of minor importance in the article reviewed. The need of a method logically sure for the determination of sensitivity was the particular point of emphasis and it was with reference to this point especially, which we believe is at present the most important in the laying of the groundwork of a more scientific psychophysics of vision, that our recommendation of energy measurements was made. In this connection it may be noted further that the article reviewed is in part a criticism of the reviewer's own advice that the eye may be used as a substitute for the nonselective instruments in the measurement of light energy; and that our criticism of this recommendation was not by any means based alone on the inadequacy of the "extant visibility data." These points are discussed in greater detail in two articles (Titchener Commemorative Volume, pp. 230–308 and *Psychological Monographs*, No. 103) which were not reviewed by Dr. Troland.

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THURSTONE'S MEASURES OF VARIABILITY IN LEARNING

As an illustration of the seriousness of our present erroneous attitude toward the meaning of learning curves, the article by Dr. Thurstone in a recent number of the BULLETIN[1] is interesting. This is especially true in view of the careful mathematical training and habits of this investigator. In the article here indicated, reporting an analysis of the learning of telegraphy by 165 drafted men, it is discovered that the variability of the pooled results of the group of learners "increases with practice." These results are expressed in the number of words received per minute, and the increase in variability with practice is shown graphically both for the semi-interquartile range and for the standard deviation. "This," says the author, "is perhaps to be expected anyway by common sense for if a frequency surface of attainment be plotted at each successive stage of practice, the range should increase with practice

[1] THURSTONE, L. L.: Variability in Learning, PSYCHOL. BULL., 1918, 15, 210–212.

while the frequencies should decrease to keep the areas of the curves constant. *Hence any measure of variability must increase with practice. Learners separate from each other more and more in attainment as they progress* " (p. 211). These last two sentences, which I have italicized, make statements which are questionable, statements which are true only for the absolute changes in the type of curve that I have called the average attainment curve (the "*a*-curve"), or for the data which such curves represent.[1] Such statements are not true for time or for error curves (the "*e*-curves"). In the case of the *e*-curves or of data expressed in errors or in time, as defined in the article referred to, the reverse is true: measures of variability must decrease with practice, and learners approach each other more and more as they progress. This is obvious from any study of learning curves that shows the simultaneous progress of several subjects,[2] if the curves represent changes in absolute units. The following curves, Fig. 1 and Fig. 2, are illustrative. They represent the progress of six subjects in a code-learning experiment carried out by the writer, with the aid of some advanced students, in the Jesup Psychological Laboratory. The subject in each case copied a continued story daily for exactly twelve minutes each period, M, P, and J writing for each letter in the words of the story its number in the alphabet; *e. g.*, "and" would be written 1–14–4. The other subjects, under precisely similar conditions, wrote the same story in the code used by the Southern Army in the Civil War, a code now used in the Goddard and the Terman revision of the Binet-Simon tests. The subject in each type of experiment was not permitted to have the code before him, but was to reproduce it ideally. Those subjects substituting numbers were frequently seen to employ their fingers in finding the correct number; but both these and the other subjects soon learned to establish certain points for orientation, as e = 5, j = 10, o = 15, t = 20. The two codes were found to be of about equal difficulty. Each subject practiced in a separate room in the laboratory occupied by himself and the experimenter, who read to him as needed the words of the story and marked successive seconds on the printed copy. Long words were spelled to the subject in the early part of the experiment so that the keeping of many letters in mind would not be distracting. Later on, the story was

[1] PETERSON, J.: Experiments in Ball-Tossing: The Significance of Learning Curves, *J. of Exp. Psychol.*, 1917, 2, 178–224.

[2] See, for example, WELLS, F. L., The Relation of Practice to Individual Differences, *Amer. J. of Psychol.*, 1912, 23, 75–78, for divergences of the *a*-curves; and PETERSON, J., *op. cit.*, pp. 188, 189, for convergences of the *e*-curves .

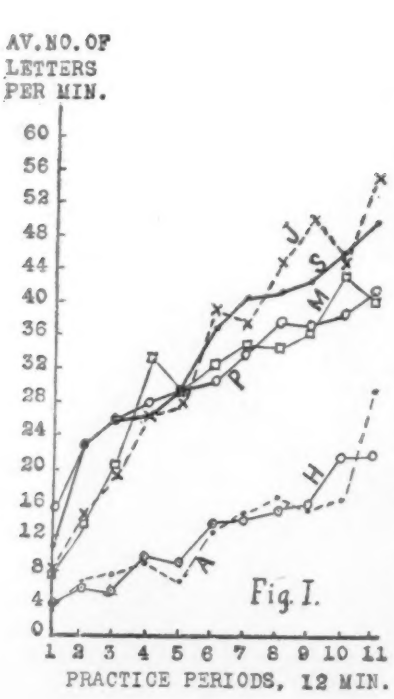
AV. NO. OF LETTERS PER MIN.
60
56
52
48
44
40
36
32
28
24
20
16
12
8
4
0
J
S
M
P
H
A
1 2 3 4 5 6 7 8 9 10 11
PRACTICE PERIODS, 12 MIN.
Fig. I.

FIG. 1

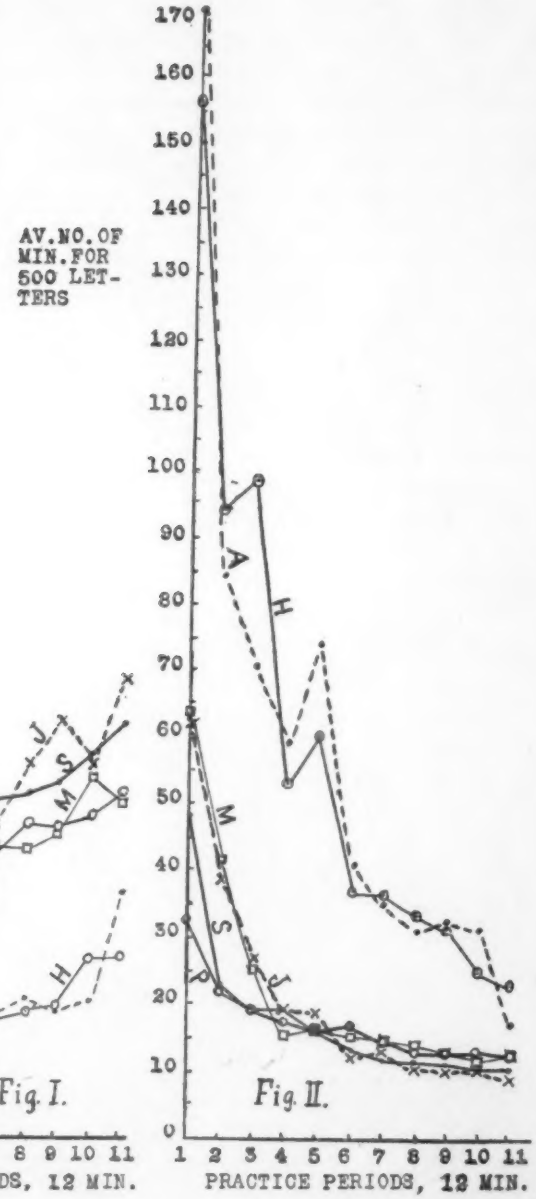
AV. NO. OF MIN. FOR 500 LETTERS
170
160
150
140
130
120
110
100
90
80
70
60
50
40
30
20
10
0
A
H
M
P
S
J
1 2 3 4 5 6 7 8 9 10 11
PRACTICE PERIODS, 12 MIN.
Fig. II.

FIG. 2

read by phrases, as the translation got more mechanical and the subject's attention went to the meaning. The subject was apprised each day of his errors in the preceding practice, and errors were not counted in the score. The subjects made very few errors after the first two days' practice.

Only correct translations were counted, every letter that was wrong being omitted in the count. Unfortunately the experiment could not be continued further, but the general trend is evident, though it is probable that the curves in Fig. 1 might again converge somewhat toward the so-called physiological limit.

In brief, the cause of the difference in trend of variability between *a*- and *e*-curves is, that average attainment always starts with small *absolute* amounts and changes toward larger absolute amounts, while the time to do a given amount of work, or the number of errors made in doing it, starts in large absolute amounts and changes toward smaller amounts. As the one type of measurement increases the other decreases; they are reciprocal relations, as I have shown in the article already cited. Any interpretation, therefore, based upon the absolute units ordinarily shown in graphs is liable to be erroneous, as in this case.

There are also questionable assumptions as to the equality of practice in successive practice periods, the effects of which on the curves we cannot here consider. If equal time practice periods are given we assume, probably wrongly, that the subjects get as much practice doing a little work in the beginning as he gets doing a great deal more, of the kind attempted, toward the end of the experiment. Roughly speaking, Dr. Thurstone's subjects seem to have averaged about 150 words each in the twenty-first period and about 390 in the seventy-second period. Are we sure that this is *equal* practice?

We may or may not grant that practice for *equal time periods* means equal periods of practice with varying degrees of ability, but there is still an obvious error in the conclusion under consideration, an error closely related to that pointed out above. Does the variability of the group, expressed either as the quartile range or as the standard deviation, increase with practice? The author concludes in the affirmative: "The variability evidently increases with practice" (p. 211). This statement is twice made, once interpreting the quartile range graph and once the graph of the standard deviation. Taking the author's smoothed curves, a rough estimate shows that the median number of words per minute

for the group is about 2.5 in the 21st hour and 6.4 in the 72d; that the quartile is about 1.5 in the 21st hour and 2.5 in the 72d; that the standard deviation is about 1.85 in the 21st hour and 3.3 in the 72d.

Therefore the coefficients of variability of the last two measures are .60 and .74, respectively, in the 21st hour and .39 and .52 in the 72d. Both of these measures, then, actually show a *decrease* in the coefficient of variability with practice, contrary to the conclusion drawn from the changes in absolute units shown on the graphs.

These facts illustrate how careful one must be in basing interpretations of changes in learning on graphs in absolute units. Mr. Thurstone's carefully plotted curves would seem to lend support, so far as they go, to my own contention elsewhere, that most cases of learning when properly interpreted probably advance about uniformly if application is kept constant and practice periods are made equal, whatever such equality may be found to mean. It is a question whether this is a legitimate inference from the graphs under consideration, and whether the author would agree to it.

It is regrettable that the subjects were removed before Mr. Thurstone completed his interesting experiment. We undoubtedly need some careful work done on plateaus and physiological limits in this type of learning.

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AN EASY METHOD OF DETERMINING THE COEFFICIENT OF CORRELATION

The tediousness of computing correlation coefficients by Pearson's "products moment" method has deterred many from undertaking investigations involving this puttery work. The figuring is, in a long problem, an Herculean task, demanding the utmost care and concentration. The necessity for such painstaking effort arises from two main sources; first, the necessity for taking account of algebraic signs; and, second, the considerable number of arithmetical computations which are demanded.

Thurstone (PSYCHOL. BULL., 14, 28–32) has already presented a method by which the first difficulty is obviated, for in his adaptation of Pearson's formula, no attention need be paid to algrebraic signs unless they are indigenous to the experimental results themselves. Methods for reducing arithmetical computation have been suggested and a description of them may be found in practically any text on Statistical Methods. But as far as I have been able

to find in the psychological and educational literature, the two methods have never been put together. The combined method results in a considerable saving of time and energy. For this reason, a brief description of it will be pertinent.

1. The first step is to make a correlation table (see Table I).

TABLE I

X ARRAY

Accuracy of mind

V	Class 2	3	4	5	6	7	8	9	f_y	$f_y \cdot V$	$f_y \cdot V^2$
y array concentration. 2	2	3	5	2	1				13	26	52
3			4	5	5				14	42	126
4	1	1	19	28	12	1		1	63	252	1,008
5		1	9	62	32	11	5	4	124	620	3,100
6		1	2	28	22	24	3	3	83	498	2,988
7			2	7	18	22	11	5	65	455	3,185
8			1	4	7	17	23	5	57	456	3,648
9				7	1	8	9	20	45	405	3,645
f_x	3	6	42	143	98	83	51	38	464 = *n*	2,754 = Σ*y* 5.935 = Av. *y*	17,752 = Σ*y*²
$f_x \cdot V$	6	18	168	715	588	581	408	342	2,826 = Σ*x* 6.091 = Av. *x*.		
$f_x \cdot V^2$	12	54	672	3,575	3,528	4,067	3,264	3,078	18,250 = Σ*x*²		

For convenience of reference, the *x* array is given in the columns and the *y* array in the rows.

2. The second step is to determine the frequency of the *x* array (f_x) by obtaining the sum of each column and the frequency of the *y* array (f_y) by obtaining the sum of each row. $\Sigma f_x = \Sigma f_y = n$, the number of measurements that have been entered in the table.

3. The third step is to multiply the frequency of each *x* column by the value (*V*) given at the top of the column. These values are given in Table I in the row labeled $f_x \cdot V$. $\Sigma f_x \cdot V = \Sigma x$ and Σx divided by *n* gives the mean or average of *x*. In a similar way Σy and the average of *y* are determined.

4. The fourth step is to obtain values for Σx^2 and Σy^2. This is done by multiplying the frequency for each *x* column by V^2, or as is usually easier, by multiplying $f_x \cdot V$ by *V*, and the result will be $f_x \cdot V^2$. The sum of these products ($\Sigma f_x \cdot V^2$) equalls Σx^2. In a similar manner the value Σy^2 is determined.

5. The fifth step is the determination of Σxy and this is the

only tedious part of the whole process. A simple method of determining this value is given in Table II. Each value of y, appearing to the left of the table, is multiplied by the sum of the values of x times the frequency appearing in that particular row. For example, consider the top row of the table. The y value is 2. Under the x value of 2 is found the frequency 2. These are multiplied,

TABLE II

DETERMINATION OF Σxy

	2	3	4	5	6	7	8	9	
2x	4+	9+	20+	10+	6			=	98
3x			16+	25+	30			=	213
4x	2+	3+	76+	140+	72+	7+		9=	1,236
5x		3+	36+	310+	192+	77+	40+	36=	3,470
6x		3+	8+	140+	132+	168+	24+	27=	3,012
7x			8+	35+	108+	154+	88+	45=	3,066
8x			4+	20+	42+	119+	184+	45=	3,312
9x				35+	6+	56+	72+	180=	3,141
									17,548=Σxy.

2×2, and the result set down in the corresponding square, then 3×3 is added to the first figure, and so on. The values for the entire first row are as follows:

$$2 \times (2 \times 2 + 3 \times 3 + 4 \times 5 + 5 \times 2 + 6 \times 1) = 98.$$

When a similar sum for each row has been obtained and these sums added, the desired Σxy has been obtained.

6. The sixth step is the solution of the formula given by Thurstone:

$$r = \frac{\Sigma xy - n \cdot \text{Av. } x(\text{Av. } y)}{(\Sigma x^2 - n \cdot (\text{Av. } x)^2)^{-2} (\Sigma y^2 - n \cdot (\text{Av. } y)^2)^{-2}}$$

Substituting, the equation becomes:

$$r = \frac{17548 - 464 \times 6.091 \times 5.935}{(18250 - 464 \times 6.091^2)^{-2}(17752 - 464 \times 5.935^2)^{-2}}$$

$$= \frac{17548 - 16774}{(18250 - 17214)^{-2}(17752 - 16344)^{-2}} = \frac{774}{(1036)^{-2}(1408)^{-2}}$$

$$= \frac{774}{1208} = +.641.$$

Futhermore, the standard deviation (σ) of the x array and the y array is easily determined from the data obtained in different parts of the problem. According to Davenport (*Statistical Methods* page 26), $\sigma = (\mu_2)^{-2}$, $\mu_2 = \nu_2 - \nu_1^2$, $\nu_1 = f \cdot V/n$, $\nu_2 = f \cdot V^2/n$. In the x array,

$$\nu_1 = 2826/464 = 6.091,$$

$$\nu_2 = 18250/464 = 39.4,$$

$$\sigma_x = (39.4 - 6.091^2)^{-2} = (39.4 - 37.1)^{-2} = 1.18$$

A much easier solution is obtained by considering the denominator of the fraction given by Thurstone's formula, for

$$\left(\frac{\Sigma x^2 - n(\text{Av. } x)^2}{n}\right)^{-2} = \sigma_x.$$

In this particular example, then, $\sigma_x = (1036/464)^{-2} = 1.18$.

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THE EDITOR

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Edited by G. Stanley Hall, John Wallace Baird and L. R. Geissler.